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Give the Null Hypothesis a Chance

Reasons to Remain Doubtful about the Existence of Psi

Is there a world beyond the senses? Can we perceive future events before they occur? Is it possible to communicate with others without need of our complex sensory-perceptual apparatus that has evolved over hundreds of millions of years? Can our minds/souls/personalities leave our bodies and operate with all the knowledge and information-processing ability that is normally dependent upon the physical brain? Do our personalities survive physical death?

Experience suggests to many people that the answer to such questions is 'Yes'. Indeed, year after year, surveys show that the majority of people believe that such paranormal phenomena exist, and personal experience is one of the primary reasons for their belief. Many such experiences are emotionally powerful and bring with them meaning and existential comfort.

What accounts for these reported experiences? Do they really, some of them at least, reflect a reality beyond the materialistic world as it is now understood by science — that is, are they really 'paranormal'? Or are they the product of normal but misunderstood brain function? That is, do our brains sometimes produce or interpret experiences in such a way that they seem to be paranormal even though they are not? Parapsychologists are motivated by and large by the former interpretation, and seek scientific evidence to support that view. Mainstream science, on the other hand, takes the latter stand, usually rejecting out of hand any paranormal claims.

Whatever the explanation, given that these experiences appear to be relatively common and are often very striking, they merit study in their own right. Unfortunately, such study is rather rare. Most psychologists, eschewing paranormal and supernatural claims, have by and large ignored such experiences, while parapsychologists, on the other hand, give scant attention to normal explanations and focus instead on the paranormal possibilities. Thus, what should be of common interest to both psychologists and parapsychologists instead falls through the cracks, with one camp persuaded that the paranormal is real and the explanation

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for many such experiences, and the other camp rejecting the paranormal while also ignoring the experience.

As a result, parapsychologists and sceptical scientists most often speak to each other in a *dialogue aux sourds*, a dialogue of the deaf. Yet, it is always a good thing to try to build bridges in the hope of bringing intellectual protagonists together, and this special issue of the *Journal of Consciousness Studies*, which includes articles by some of the leading proponents and critics of parapsychology, may help build such a bridge. As much as they may differ in terms of their views on the paranormal, it is important to note that the contributors are 'all on the same side' in at least one important way: all share a deep respect for science and are committed to the scientific method as the appropriate approach to exploring reality. They are all seeking truth, not delusion; fact, not fiction. Arguably, the only significant differences that distinguish the proponents from the sceptics in this collection of articles are in terms of their *a priori* subjective weighings of the likelihood that psychic phenomena exist, which in turn may influence their evaluations of the adequacy of the research protocols that have been employed in parapsychological research and the quality of the data thus obtained.

Those in the scientific community who have little familiarity with parapsychology are often unaware of the wide spectrum of opinion, expertise and degree of respect for science, that exists amongst those who call themselves parapsychologists. At one end are those described in the last paragraph, of whom some have contributed to this volume. At the other are numerous writers and researchers who view science as an inadequate tool for grappling with the mysteries of the paranormal, and who base their beliefs in the paranormal solely on the kinds of experiences served up by trance mediums, putative apparitions, and so forth. Their writings are not to be found in this Special Issue, nor are the writings of those who believe that the verdict is already in, that parapsychology has long since established a sound scientific footing for paranormal phenomena and no controversy remains. (Indeed, this touches on a demarcation problem, in that scholarly, research-oriented parapsychologists reserve the label of 'parapsychologist' for themselves, and do not consider members of the general public who use this title to be parapsychologists. This important distinction is often difficult to make for those outside parapsychology.)

There is also a spectrum of opinion, expertise, and yes, of the degree of respect for science, amongst sceptics too, and again, those at the far end who only sneer dismissively at any mention of the paranormal, or those whose dogmatism shows an inability or unwillingness to be objective, are not to be found in this Special Issue.

Thus, to the sceptical reader, I stress that these parapsychological writers are in our camp, the scientific camp. They believe in science and strive to apply it. To the reader who leans towards belief in the paranormal, the sceptical writers you will find here are not motivated by any desire to drive parapsychology into the desert, but only by the desire to truly understand human experience.

That being said, I have myself long been a critic of parapsychological research, and it is only fair that I state my views 'up front'. I have yet to find any

empirical evidence that persuades me that it is likely that paranormal phenomena actually exist. Moreover, I am well aware of just how often our brains can mislead us, and can lead us to believe that we have had a paranormal experience even when no such thing has happened. Indeed, even if there is no such thing as a paranormal phenomenon, human information processing works in such a way that we are all likely from time to time to have experiences that *seem* for all the world to be paranormal. For me as a psychologist, these experiences themselves — the reports of extrasensory perception and the like — are fascinating in their own right, even if, as I presume, they are not paranormal, for they can tell us a great deal about how our brains work and about our beliefs and needs and expectations, if we are willing to listen.

I approached my own reading of the articles in this Special Issue in part with the personal desire to find out if there is any new and compelling evidence that might nudge me away from my strong scepticism about the existence of paranormal phenomena. There are for me a number of reasons to be doubtful about the existence of paranormal phenomena (I shall adumbrate some of the more important ones below), and thus, I perused each article against the backdrop of those concerns, and considered whether its conclusions supported the Psi hypothesis (that psychic, or 'psi', phenomena exist), or were they more in line with the Null hypothesis (that is, that the observed results came about naturally, and had nothing to do with psi). I advocate that the reader take a similar approach, keeping in mind not just the Psi-hypothesis, but the Null hypothesis as well.

Reasons to Remain Doubtful about the Existence of Psi

1. Lack of definition of subject matter

One of parapsychology's most vexing problems has to do with the very definition of its subject matter. What is it that is being studied, and how are the phenomena under study themselves defined? Are ghosts, levitation and trance channellers part of the accepted range of subject matter? Or is the subject matter at this time restricted to subtle mind-induced influences at the micro level? If mainstream science is challenged to consider seriously the claims of parapsychology, just what claims are we talking about — ghost sightings, or small but statistically significant changes in a distribution of outcomes of a random event generator in a laboratory experiment? Parapsychological opinion as to its proper subject matter varies widely. Consider, for example, the views of the following organizations:

The Parapsychological Association (PA) defines itself as 'the international professional organization of scientists and scholars engaged in the study of "psi" (or "psychic") experiences, such as telepathy, clairvoyance, remote viewing, psychokinesis, psychic healing, and precognition . . .', and its webpage states:

> The diversity found within PA membership also leads to many different 'schools of thought' regarding the phenomena studied — ranging from those who suspect that psi will eventually turn out to be an artifact of no major significance, to those who

> believe it will be accounted for through new developments in physics or biology, to those who argue that psi phenomena suggest a basis for spiritual beliefs.
>
> (*www.parapsych.org*)

In Britain, the venerable Society for Psychical Research states that:

> The principal areas of study of psychical research concern exchanges between minds, or between minds and the environment, which are not dealt with by current orthodox science. This is a large area, incorporating such topics as extrasensory perception (telepathy, clairvoyance, precognition and retrocognition), psychokinesis (paranormal effects on physical objects, including poltergeist phenomena), near-death and out-of-the-body experiences, apparitions, hauntings, hypnotic regression and paranormal healing. One of the Society's aims has been to examine the question of whether we survive bodily death, by evaluating the evidence provided by mediumship, apparitions of the dead and reincarnation studies.
>
> (*www.spr.ac.uk/about.html*)

The American Society for Psychical Research (*www.aspr.com/topics.htm*), which describes itself as the oldest psychical research institute in the United States, lists as its subject matter an extensive range of topics, from extrasensory perception to psychic healing to trance channellers and survival after death to dowsing and poltergeists.

The point is that there is a great variety of opinion as to what constitutes the essential and appropriate subject matter of parapsychology. Some parapsychologists want to adhere to the rules of evidence as they exist in modern science, while others rely on anecdotal accounts of wondrous events — such as the supposed levitation of the medium Daniel Douglas Home during a seance in 1852 — as the best evidence that psi phenomena are real. Recently, some prominent members of the Society for Psychical Research have become very interested in the study of spirit mediums once again, based on sittings in Scole, England, and view this evidence as strongly suggestive of communication with a spirit world (Keen, Ellison and Fontana, 1999). Yet, such research would probably be considered quaint and unscientific by more laboratory-oriented parapsychologists such as those whose articles appear herein, and such research, despite the importance it is given in some parapsychological circles, is not even mentioned by the parapsychologists who have contributed to this Special Issue.

This all reflects the fact that to the extent that parapsychology constitutes a 'field' of research, it is a field without a core knowledge base, a core set of constructs, a core set of methodologies, and a core set of accepted and demonstrable phenomena that all parapsychologists accept. Moreover, I consider it doubtful that parapsychologists could agree amongst themselves as to just what experiments or demonstrations in the literature constitute the best case for psi. This immediately distinguishes parapsychology from any other scientific research field, where there is always a common core of knowledge as well as key demonstrations that can reliably be produced and taught, even while there may be controversy about various concepts and research findings at the frontiers of the field.

2. Definition of constructs

Quite apart from differences of viewpoint in what constitutes the range of appropriate subject matter, a much more important definitional problem arises in terms of defining and measuring specific psi phenomena. The problem arises primarily because psi phenomena are defined, not in terms of what they are, but only in terms of what they are not. Telepathy is the simultaneous sharing or transfer of information between two brains *in the absence of any 'normal' mechanism that could account for it*; precognition involves seeing future events *in a manner that cannot be accounted for by any means understood by contemporary science*, and so on. Telepathy is not telepathy if sender and receiver communicate by 'silent' dog whistles that one of them is able to hear, or if they have some sort of secret code that allows them to communicate without the knowledge of the researcher. Psychokinesis is not psychokinesis if the psychic causes an object to move by hidden, although normal, means. Indeed, parapsychology is the only realm of objective inquiry in which the phenomena are all *negatively* defined, defined in terms of ruling out normal explanations. Of course, ruling out all normal explanations is not an easy task. We may not be aware of all possible normal explanations, or we may be deceived by our subjects, or we may deceive ourselves.

If all normal explanations actually could be ruled out, just what is it that is at play? What is *psi*? Unfortunately, it is just a label. It has no substantive definition that goes beyond saying that all normal explanations have apparently been eliminated. Of course, parapsychologists generally presume that it has something to do with some ability of the mind to transcend the laws of nature as we know them, but all that is so vague as to be unhelpful in any scientific exploration. Some parapsychologists, recognizing the problem of trying to provide a positive rather than a negative definition of psi, choose to sidestep the issue and instead focus on 'anomalies'. Psi effects are thus thought of as anomalous findings that apparently should not occur if the current scientific worldview is accurate. These are not just any such anomalies, of course. They are anomalies that involve, in one way or another, the mind.

Anomalistic observations that do not fit with accepted theory are vital to scientific progress, for they force us to modify our theories and to gather additional data until they can be understood and accommodated into a revised theory. For example, to AIDS researchers it is quite anomalous that some Nairobi prostitutes show an inherent resistance to HIV infection, but only as long as they continue to have exposure to multiple partners. This is an important anomaly — it does not make immediate sense in terms of what is known about this illness, but coming to understand it will undoubtedly lead to a much better understanding of HIV in general. Elsewhere in science, anomalies sometimes lead to such fundamental changes in theory that philosophers of science speak in terms of a paradigm shift. The precession of Mercury in its orbit behind the sun was anomalous; for it did not fit with Newton's theory of gravity and the derivative understanding of the movement of planets. Scientists a century ago went so far as to speculate that Mercury's orbit behind the sun was actually disrupted by the gravitational field of an unseen planet (they called it Vulcan) on the far side of the sun. However,

Einstein's general theory of relativity was able to account for the perihelion shift of Mercury, resolving the anomaly and thereby helping to usher in a new scientific worldview.

Yet, when parapsychologists seek to establish their subject matter in terms of anomalies, there is something quite different going on compared to either of the examples above. In mainstream science, one does not deliberately seek anomalies; they present themselves. They are unexpected and unpredicted by current theory, that is why, after all, they are called anomalies. However, no psi anomaly has ever presented itself in the course of research in mainstream science. Consider the particularly delicate experiments in subatomic physics, which might be ideal for the manifestation of putative psi forces, given that they involve very tiny amounts of matter and energy, highly precise measurements and very highly motivated researchers with, at least at times, varying expectations. We do not read research reports that suggest that the outcomes of such experiments seem to depend on who was operating the linear accelerator at the time, and that a particular effect is found only when certain researchers are present and not otherwise, reflecting perhaps a researcher's 'psychic' influence. In the course of doing normal science, anomalies suggestive of psi just do not pop up. Rather, parapsychologists, in their work, deliberately try to generate them; they are the goal of much parapsychological research and are only labelled as anomalous by the rather circular route of deeming them to be impossible if current science is accurate and complete.

Parapsychologists need to be able to provide a positive definition of psi, to tell us how to identify psi 'anomalies' in ways other than exclusion, and to tell us how to rule out psi, how to know when it is absent. This problem is as great now as it has ever been, and no progress has been made in overcoming it across more than a century of empirical parapsychological research. Because of its negative definition, we are left with no idea as to when psi might occur, and more importantly to the scientist, as to when it will *not* occur. There is no way, we are told, that psi can be blocked or attenuated by the researcher, and thus we cannot compare conditions where psi could not occur to those where, were it to exist, it could be observed. Moreover, because it is claimed that psi influences can occur without any attenuation as a function of distance, and can occur backwards and forwards in time, it becomes impossible ever to truly 'control' the conditions of an experiment.

3. Failure to achieve replication

If parapsychologists cannot provide a positive definition of psi, then at least one would hope that they could provide a reliable, replicable, demonstration of the subject of their study, be it an 'anomaly' or whatever. Mainstream science accords a high value to replicability, for it is perhaps the best safeguard against being taken in by results produced by error, self-delusion or fraud. Yet replicability itself is a somewhat complex concept. Simply repeating an experiment and getting the same results is not by itself enough, for whatever errors or

self-delusions may have occurred in the first instance might also be part of subsequent repetitions of the experiment (Hyman, 1977). That was precisely the case when, at the beginning of the twentieth century, the French physicist, Professor Blondlot, 'discovered' N-rays, an apparently new form of energy. He replicated his experiments many times, and indeed, a score or more of other scientists reported that they had confirmed the existence of N-rays in their own laboratories. Yet sceptical scientists were unable to replicate these results, and ultimately Blondlot's findings were shown to be a product of self-delusion (Alcock, 1981). The concept of replicability, to be useful, implies that researchers in general, provided that they have the expertise and equipment, should be able to reproduce the reported results, and not just those who are believers and enthusiasts.

Because parapsychologists have *never* been able to produce a successful experiment that neutral scientists, with the appropriate skill, knowledge and equipment, can replicate, some parapsychologists have gone so far as to argue that the criterion of replicability should not be applied to psi research because the phenomena are so different from the usual subject matter of science (Pratt, 1974). Yet, what a risky adventure it would be to yield to special pleading and relax the very rules of scientific methodology that help to weed out error, self-delusion and fraud in order to admit claims that violate the basic tenets of science as we know it!

Several of the papers in this Special Issue address the problem of replicability in psi research:

(1) My good and respected friend Adrian Parker acknowledges the highly problematic inconsistencies in parapsychology that reflect both failures to replicate and situations where some experimenters, but not others, can replicate a set of findings. Yet he does not take this to suggest that the Psi hypothesis might be wrong and the Null hypothesis correct, but instead views these irregularities as reflecting possible *properties* of the ostensible phenomenon, such as the *psi-experimenter effect* (discussed below). This is begging the question. When there has been a failure to replicate, it is not appropriate to engage in the circularity of assigning to this failure a label (psi-experimenter effect), and then implicitly suggesting the label as its explanation. Since there is no other way of defining or identifying the psi-experimenter effect, it has no *explanatory* value. Using it as a possible explanation only leads to a tautology: by substituting the definition of the psi-experimenter effect, one gets 'The failure to replicate may be a manifestation of "one researcher failing to replicate a finding that another researcher had made".' This circular reasoning excludes from the debate a possibly fruitful aspect of research, in terms of coming to understand the reasons, other than psi, that might account for the fact that different experimenters have obtained different results.

(2) With regard to ESP in the ganzfeld, Palmer concludes that, while he finds statistically significant departures from the Null hypothesis across the aggregate data bases that he has examined, 'the marked heterogeneity of results across experiments leaves doubt about the future replicability of the phenomenon outside parapsychology'.

(3) In their article, Sherwood and Roe examine attempts to replicate the well-known Maimonides dream studies that began in the 1960s. They provide a good review of these studies of dream telepathy and clairvoyance, but if one thing emerges for me from their review, it is the extreme messiness of the data adduced. Lack of replication is rampant. While one would normally expect that continuing scientific scrutiny of a phenomenon should lead to stronger effect sizes as one learns more about the subject matter and refines the methodology, this is apparently not the case with this research. They conclude: 'Overall, the Maimonides studies were more successful than the post-Maimonides studies but this may be due to procedural differences.' Indeed, this leads the authors to indicate that 'more recent work has concentrated on the question of whether consensus methods are superior to individual performance. With consensus judgement procedures, the responses from a number of individuals are combined to give a single judgement.' To the sceptic, this is a strange turn of events. The phenomenon of interest is the alleged ability of some individuals to paranormally receive information while they are asleep. Because research cannot demonstrate this clearly, the researchers choose to complicate the situation immensely by combining information from a number of subjects.

(4) Jeffers' article also bears directly on the question of replicability. Jeffers stands in lonely company as one of the very few neutral scientists who have empirically investigated the existence of psi phenomena. My first interaction with Jeffers is memorable to me. Jeffers, a physics professor at my university, was inspired by the work of Robert Jahn (e.g. Jahn, 1982), that purported to demonstrate the influence of the human mind on the output of a random event generator, and he decided to carry out his own psi experiments. His methodology was different from Jahn's (or indeed from other psi experiments) in that it investigated the possible effect of psi on the interference of light. He reasoned, and Jahn had agreed, that if Jahn's results were due to subjects' mental influence on quantum processes, then that same influence might be expected to affect the interference patterns produced when two beams of light are sent through narrow slits. In Jahn's work, a series of numbers appeared on a computer screen, the ultimate result of a quantum process, and subjects strove to affect the magnitude of those numbers. In Jeffers' work, a bar appeared on a computer screen, its length determined by a quantum process (fringe contrast in the interference pattern) and subjects attempted to influence the height of the bar. Thus, Jahn and Jeffers were both attempting to measure subjects' ability to influence quantum processes by mentation alone and, given that different methodology was used, were Jeffers' research to have produced significant results this would have added even more weight to Jahn's conclusions than would a straight replication. This is because Jeffers studied the same construct, or concept, from a slightly different angle, thereby making his research capable of producing convergent evidence, whereas a straight replication using exactly the same methodology might also reproduce any undetected errors and biases in the original.

Back to our initial meeting: Jeffers came to me at least a tad defiantly, requesting that I review his experimental design and offer any suggestions and

criticisms before he began his research. He stressed that I should not after the fact, were he to obtain data supporting the parapsychological interpretation, then argue that the experiment was not to be taken seriously because it had fallen methodologically short in some fashion. Thus began our relationship, which was to grow into the very positive one that it is today. I reviewed his experimental design, and I raised some reservations — the same reservations that I had written about (Alcock, 1990) with regard to Jahn's work. While so far as I am aware, Jahn's group never paid any heed to my comments, Jeffers incorporated changes that satisfied all my concerns. As Jeffers reports in his paper, his research findings give no support to the Psi hypothesis.

Jeffers' research makes a very important contribution to the study of putative psi phenomena, in my opinion, for the following reasons:

1. It was carried out by a neutral scientist who approached the subject with great interest and motivated by the possibility that Jahn may really have discovered something very important — the influence of human mentation on random physical processes. This should be an ideal condition for producing the desired results: Jeffers was very much open to the possibility of psi and was motivated to find it.
2. The research began with the full approbation of both proponent and sceptic. Jeffers' had the full-fledged support of Jahn himself and, as noted above, I fully supported the appropriateness of the revised methodology that he employed. Had he produced positive results, Jahn no doubt would have viewed this as a significant conceptual replication of his own work by a neutral scientist, and I in turn would have had to admit that the research was done carefully and correctly, and that I had no basis for rejecting it on methodological grounds.

However, when Jeffers' research did not produce results supportive of the Psi hypothesis, other researchers in the area dismissed it, and now it receives virtually no attention from parapsychology at all. (To be precise, his article discusses two kinds of experiments, one single-slit and one double-slit. The results of the single-slit experiment, carried out at York University, were null. There were two sets of double-slit experiments, one conducted at York University and one carried out in Jahn's laboratory at Princeton. The York experiment produced a null outcome, while that at Princeton produced 'marginal' significance ($p = 0.05$), which Jeffers views, as do I, as unconvincing). This neglect of Jeffers' research is most unfortunate. Although his data, as reviewed in his current paper, is in line with the Null hypothesis, the fact that it is now ignored within parapsychology is another instance of not giving the Null hypothesis a fair chance.

Incidentally, Jahn's laboratory more recently collaborated with researchers at two German universities to attempt a carefully controlled replication of the basic claims of Jahn's research group. The result? Neither the researchers at Jahn's lab nor those in the two German universities found anything of significance with regard to the hypotheses under test (Jahn *et al*., 2000). They did, however, on a *post-hoc* basis — as is so often the case in parapsychology —

find some 'anomalies' in the patterning of the data which they argue call for more sophisticated experiments and theoretical models in order to understand 'the basic phenomena involved'. Again, failure to confirm predictions does not, in their view, give strength to the Null hypothesis. By post-hoc data snooping, a success of sorts can always be wrestled away from the jaws of the Null.

In sum, parapsychologists have never been able to produce a demonstration that can be reliably replicated by researchers in general, and failures to replicate are either ignored, explained away or interpreted as evidence for the existence of arbitrary properties of psi, as is discussed below.

4. Multiplication of entities

Despite William of Ockham's exhortation that one should not increase the number of entities required to explain a phenomenon beyond what is necessary ('Ockham's Razor'), parapsychology has unabashedly invented a number of such entities by way of explaining away failures to produce consistent and replicable data. For example:

1. As touched on earlier, if only some researchers can obtain an effect — and then only some of the time — while other researchers using identical methods cannot, this is taken, not as lending support to the Null hypothesis, but as a manifestation of a property of psi — the *psi-experimenter effect.* This 'effect' supposedly occurs because some experimenters, perhaps because of their own psi abilities, are conducive to the production of psi in experiments, while others are not.

   Smith's article in this Special Issue provides a good overview of the enduring problem of the experimenter effect in parapsychology, but his analysis also indirectly serves to demonstrate the problem that I am addressing. While acknowledging the issue of replication in parapsychology, Smith argues that 'replication difficulties in parapsychology may be due, at least in part, to psi-related experimenter influences'. He recognizes that this view is difficult from the point of view of science because it suggests that 'it is only those researchers who believe that psi exists that are likely to be able to replicate positive results'. Nonetheless, as he reflects upon this problem, Smith's optimism is not diminished and he argues: 'the scientific approach adopted by psi research has so far achieved some limited success in identifying factors associated with obtaining positive results in psi experiments, and it is my view that it is such an approach that is likely to reveal more of these factors in future research. Only when we have a much more detailed recipe for success can more consistent levels of replication be expected.' Thus, while aware of the problem he sidesteps it.

   Parker also addresses this subject, and states that 'experimenter effects and psi-conduciveness are every bit as integral part of the phenomena being studied as, say, placebo effects are in psychological treatment'. The problem is that the 'experimenter effect' is really only a lack of consistency, a lack of

general replicability, which itself is more in line with the Null hypothesis than anything else. There is no reason, no justification, to engage in further multiplication of explanatory entities, to use Ockham's language. What we have here is a failure to replicate. Period. The psi-experimenter effect provides the ultimate Catch-22: if you find the psi effect you are looking for, well and good. If you do not find it, this might be because of the experimenter effect, and so this too could be a manifestation of psi!

2. The *sheep–goat effect* refers to the observation that believers in psi are more likely than non-believers to demonstrate evidence of psi in an experiment.
3. If subjects fail to obtain the above-chance scores predicted in a psi experiment, that is not taken as lending weight to the Null hypothesis. Instead — so long as they fail miserably enough that their data deviate statistically significantly in the non-predicted direction, then this is taken as support for the Psi hypothesis, and another 'effect' — the *psi-missing effect* is invoked, allowing the interpretation that the miserable failure was indeed a success.
4. If a 'gifted' subject scores well in early trials but then, as is so often the case, scores only at a chance level later, this is not taken as support for the Null hypothesis. Instead, it is taken as evidence for another 'property' of psi — the *decline effect*. Thus, failure is often interpreted as a kind of success, as an indication of the weird properties that this elusive psi possesses.

I note that one such 'effect', at one time well-known within parapsychology, appears to have quietly disappeared. I am referring to the *quartile-decline effect*, much discussed by the pre-eminent parapsychologist Joseph Banks Rhine, and so-named because it was noted that when subjects' scores were recorded in two columns to a page, there was often a significant decline in subjects' success if one compared the scores in the upper left-hand quadrant of the page to those in the lower right-hand quadrant. While such an 'effect' always struck sceptical observers as somewhat convenient and arbitrary, it was touted as again suggesting some strange property of psi.

Indeed, the very fact that it has proven so difficult to produce a reliable demonstration of a psi phenomenon has led some researchers to think of this general elusiveness not as something in line with the Null hypothesis, but rather as another property of psi. Parker's paper speaks to this: 'For whatever reason the phenomena appear to have an elusiveness as a defining characteristic that makes them intrinsically difficult to capture in the laboratory in a stable, predictable and controllable fashion.'

Note that none of these so-called effects are anything other than arbitrary, post-hoc labels attached to unexpected negative outcomes. The employment of arbitrary *post hoc* constructs to explain away failures and inconsistencies in the data is a serious problem when one considers the scientific status of parapsychology. The Null hypothesis is not given a fair chance when data that are consistent with it are explained away in this manner.

5. Unfalsifiability

Obviously, the use of such 'effects' as those just discussed serves to make claims about psi essentially unfalsifiable, for any failure to produce the predicted effect, or any inconsistency in the data, can be explained away in terms of one or another of them. Failure to produce data consistent with psi has never been taken as providing weight to the null hypothesis.

Falsifiability is an important concept in science, especially when highly unusual claims are made. Science did not ignore Roentgen's rays just because they did not fit in with what was known at the time. On the other hand, science did not ignore Blondlot's rays (N-rays) either. The former turned out to be a highly replicable phenomenon that demanded changes in physical theory to account for it. The latter, despite numerous independent 'replications' initially, turned out to be a figment of the imagination. This is why falsifiability is so important.

6. Unpredictability

This problem is also related to the replication difficulty. Parapsychologists cannot in general make predictions before running experiments and then confirm them. Yet, as discussed earlier, even if predictions are not confirmed, researchers often point to some apparent irregularity in the data that suggests, *post-hoc*, that some other psi event occurred.

Yet, if psi is real, one might expect that psi manifestations would be predictable, as least to some extent. With the vast amounts of data that parapsychologists typically collect, it would be straightforward enough to calculate the number of datapoints needed to obtain an effect size of an arbitrary magnitude, and then rerun the study with that number of data points, and find the predicted effect if it is there. It never works out that way. This has led Palmer to admit to 'what appears to be an intractable problem in parapsychology. Until we can predict such outcomes ahead of time, the establishment of lawful relationships still evades us.' This unpredictability, I must point out, is what one would expect to find if the Null hypothesis, rather than the Psi hypothesis, obtains. If the Null hypothesis is true, if there is no such thing as psi, then 'significant results' occur from time to time because of a concatenation of chance factors, flaws in the experimental design, and so on. In such a case, one would not expect any lawfulness in the data, and one would not be able to predict what should occur in the next experiment based on what has happened in the last.

7. Lack of progress

Not only is there a problem of general inconsistency in the data, as discussed above, there has not been any real improvement in this situation over time. Despite the use of modern random event generators and sophisticated statistical analyses, parapsychologists are no closer to making a convincing scientific case for psi than was Joseph Banks Rhine back in the 1930s. There has been no growth in understanding. Psychic phenomena, if they exist, remain as

mysterious as ever. No consistent patterns have emerged. Effect sizes do not grow over time as a result of refinements in methodology. No well-articulated theory supported by data has been developed. Indeed, rather than producing a gradual accumulation of knowledge and an evolution of better and better methodology, every decade seems to spawn some new methodology or paradigm or research programme that offers promise of the long-awaited breakthrough, but that gradually loses its glitter. The famous Rhine experiments (e.g. see Rhine *et al*., 1966/1940) are no longer held up as strong evidence for the Psi-hypothesis. Soal's research (e.g. Soal and Bateman, 1954), once trumpeted, is now forgotten, and for good reason. Targ and Puthoff's remote-viewing experiments (e.g. Targ and Puthoff, 1974) , which showed early promise, now are virtually ignored, again for good reason. The Maimonides research has been difficult to replicate, as Sherman and Roe point out. Jahn's research group at Princeton continues its efforts (e.g. Jahn *et al*., 2000), but its impact is minimal within modern parapsychology, partly due to methodological problems identified by other parapsychologists and critics alike. There has been no real growth in understanding or in the ability to isolate the putative phenomena over time. New research strategies seem to 'fret and strut their hour upon the stage' and then are heard little more.

8. Methodological weaknesses

Given that psi is defined negatively, and can only assumed to have been present if all possible normal explanations can be ruled out, critics of parapsychology are naturally inclined to look for flaws in the experimental design and execution of research that would account for whatever positive effects parapsychologists have adduced. Of course, this quest is hampered by the fact that experimental reports will only rarely capture sources of error of which the experimenter was oblivious, and so it is not always possible in the first instance to find normal sources of putative psi effects based on the write-ups alone. The nub of the debate between sceptic and proponent is most typically the adequacy of the methodology. I think it fair to say, and I suspect that both Parker and Palmer would agree with me on this, for they have been strong methodological critics of much parapsychological research themselves, methodological weaknesses have, in a large number of studies, vitiated the claim to have demonstrated something paranormal. However, some parapsychologists have argued that even when errors and weaknesses are found, the onus is on the critic to show that the error could have produced the observed effects. That argument is not persuasive however, for the onus is always on the researcher to demonstrate that he or she has done the experiment well, and flaws in design or procedure show that it was not done well, and that perhaps other less obvious methodological problems have also been a factor. The answer is simply to run the experiment again, doing it right this time. That is what is expected in mainstream science. The problem for parapsychology, however, is that the difficulty in replication means that it may not be possible to get the same results a second time, whether the methodology is cleaned up or not.

However, are sceptics too intent on finding methodological flaws and in so doing failing to see the phenomenon of interest? One must, of course, be careful not to throw the baby out with the bathwater when one approaches data that do not fit in with the contemporary scientific worldview. There are many examples were the baby was thrown out, only to be rediscovered years later, crying out for attention. Mesmer argued that he was 'curing' hysterics by means of animal magnetism. Mainstream scientists of the day who were charged with the evaluation of his claims demonstrated that his explanation was wrong, that when the metal rods that he used in his procedure were secretly removed, this made no difference to the outcome, and his patients still responded positively to the procedure. Mesmer would not back down; he stood by his theory of animal magnetism, and the necessity of the metal rods. The scientists would not back down; they stood by their findings that magnetism had nothing to do with it. As a result, Mesmer's clinic was shut down, and both sides in the dispute missed a wonderful opportunity to discover and explore what we now call 'hypnosis'.

I agree with parapsychologists when they declare that if a single instance is known in which 'action at a distance' occurred, we at least know that contemporary science does not encompass the whole story about nature. Yet, if the observed action at a distance is not replicable, then it is questionable whether it has really been demonstrated to occur. Indeed, it is important to remember that hypnosis was ultimately 'discovered', though its true nature remains subject to some debate even today. While not everyone appears to be susceptible to hypnosis, just about anyone can quickly learn to produce hypnosis in susceptible subjects simply by following a standard script. In comparison, over a century of parapsychological inquiry has as yet failed to produce a publicly replicable demonstration of psi, and that despite its long history, parapsychology still lacks the evidence it needs to be placed before the scientific community for judgment. (Hyman, 1977; 1985; 1989). At some point, it seems justifiable to presume that there may not be a baby in the bathwater, and that the Null hypothesis is correct!

9. Reliance on statistical decision-making

Because of the failure to be able to produce a straightforward demonstration of psi ability, such as might be the case if a psychic could reliably predict winning lottery numbers, or if, as Gardner (1957) suggested many years ago, a psychic could cause a fine needle, which is carefully balanced on another needle and housed under a Bell jar from which the air had been evacuated, to rotate, parapsychologists at the more scientific end of the spectrum came to depend more and more upon statistical analyses to demonstrate their putative phenomena. With such an approach, subjects make guesses or make mental attempts to influence random event generators, and then their success or failure is judged by a statistical comparison with what would be expected by chance alone.

Statistical analysis was applied first in psychological research as a means of protecting the researcher against error. It allowed the researcher to evaluate the likelihood that his or her results, no matter how strong the data appeared to the

naked eye, could have occurred by chance alone. In recent years, such analysis has been employed to do much more than simply provide guidance about the likelihood that particular data may well have arrived by chance alone. Powerful statistical techniques now exist for finding patterns in data that elude the naked eye, and this provides an important tool for researchers in many domains. Thus, statistical analysis originally helped cool our ardour about what appeared to be meaningful effects in the data, whereas now, those statistical tools are used to find significant effects that we would not otherwise detect. Now, in modern parapsychology (and, alas, in mainstream psychology as well to some extent), statistical analyses are being used to define and defend the importance of differences so small that they would have carried no interest to researchers of a century ago. If subjects score at a rate of 51% when the chance rate is 50%, it is unlikely that anyone would have taken any notice a century ago. Now, provided the sample size is large, such a small difference may well be 'statistically significant'.

There is no reason in principle that such analysis should not also be used in parapsychology, but there is an important difference in the way that it is used in that field. In regular science, statistics are used either to look for covariation amongst well-defined variables, or to evaluate whether a given measurement is affected by the presence or absence of an 'independent' variable. However, in parapsychology, there are no well-defined variables, and there is no way of controlling whether psi (if it exists) is present or absent, and so the statistical process is used, not to evaluate the effect of one or more variables on other measurable variables, but as a basis for inferring the presence of psi itself. One begins with the assumption that a particular mathematical distribution describes the probability distribution of outcomes of a randomly generated event. A subject in some way tries mentally to influence the distribution of outcomes (even if he or she knows nothing about the nature of that distribution, or about the generator that produces it, or even where the generator is physically located). If the outcomes depart from the theoretical distribution to a significant extent, this is taken as evidence that a psi influence caused the departure.

Any such statistically significant departure is viewed as an 'anomaly' relating to psi, and thus is viewed as support for the Psi hypothesis. However, statistical significance tells us nothing about causality. If a person tries to guess or 'intuit' what number will come next in a randomly generated sequence, and succeeds better than one would expect by chance, that tells us absolutely nothing at all with regard to why such results were obtained. The departure from chance expectation could be due to any number of influences — a non-random 'random generator', various methodological flaws, or . . . Zeus. (I could posit that Zeus exists and likes to torment parapsychologists, and thereby gives them significant outcomes from time to time, but does not allow replication outside parapsychology. The significant outcome would provide as much support for my hypothesis that Zeus exists as it does for the Psi hypothesis that the human subject's volition caused the results.)

Joseph Banks Rhine, whose psi research was motivated in part by the desire to find scientific evidence for post-mortem survival, passionately believed in the

scientific method, and consequently he shepherded parapsychology into the laboratory and into the research paradigms favoured by experimental psychologists — studies with specific targets, controlled conditions and statistical analysis of data. It was at that juncture that, despite the admirable effort to harness emerging social science technology, formal parapsychology began to lose touch with the very experiences that originally motivated its pursuit. Rather than focusing on conditions that seem to be conducive to paranormal experiences such as telepathy in everyday life, and then seeking at first to understand the experience in terms of normal psychology, these laboratory studies focused only on one explanation of such experiences — the notion that somehow there is a transfer of information that does not involve any known sensory apparatus or energy. Thus, the laboratory approach involved trying to 'send' information from one brain to another, or trying to 'read' objects hidden from view, or trying to predict the outcome of the roll of dice before they are thrown. Note that such activities have virtually nothing to do with human experiences that seem to many to be paranormal. Worse, verification of the supposed success of the psi task became a statistical one. No longer was it a question of whether a person had dreamed of his father's funeral in detail, not knowing that miles away his father had died, but rather, what is the series of cards that will next be turned up?

10. Problem of theory

That quintessential investigator Sherlock Holmes once opined: 'It is a capital mistake to theorize before one has data. Insensibly one begins to twist facts to suit theories, instead of theories to suit facts.' This is also good advice when it comes to theorizing in parapsychology. The database does not at this time justify the development of explanatory theory, for, as I have discussed above, it is far from clear that there is anything to explain. Notwithstanding the absence of good evidence, there have been many attempts to develop theories to explain putative psi phenomena, among them the Conformance Behaviour model (Stanford, 1990); Decision Augmentation Theory (May, Utts and Spottiswoode, 1995); a teleological (goal-seeking) theory (Schmidt, 1975); a quantum mechanical theory (Walker, 1984); the Thermal Fluctuation Model (Mattuck, 1982) that proposes that the 'mind' somehow alters the outcome of an event by manipulating the thermal energy of molecules; and, as described in this Issue, Pallikari's statistical balancing theory (discussed below). Such theorizing in the absence of reliable data, especially when it attempts to interpret quantum mechanical theory in such a way as to accommodate psi, lends an unjustified patina of scientific respectability to parapsychology, especially in the eyes of those who are outside the world of physics.

Jeffers' paper critically discusses the argument often heard within parapsychology that quantum physics in some way or another can accommodate/explain psi. Pallikari, on the other hand, begins with the assumption that psychokinesis occurs, and her analysis concludes that it only operates at a micro-level, and therefore does not show up at the macro level. She proposes a theoretical approach

to understanding such micro-PK, and inherent in this notion is the idea of statistical balancing in the long run, so that macro-PK will not be observed. While I admire Pallikari's efforts, they are premature, for the problem remains that to date there are no substantive empirical data to justify such theorizing. Of course, her conclusion that psychokinesis does not show up except at the micro level is at variance with what many other parapsychologists have claimed to have observed at the macro level.

11. Failure to jibe with other areas of science

A major criticism of parapsychology is that it fails to jibe with other areas of science. The late neuropsychologist Donald Hebb (1978) once commented that if parapsychology is right, then physics and biology and neuroscience are horribly wrong in some fundamental respects. He went on to say that science has been wrong before, but that parapsychology would need very strong evidence if it was going to be able to challenge successfully the current state of knowledge in mainstream science. For example, psi influences, unlike any known energy, are invariant over distance. Time produces no barrier either, apparently, for such influences are said to be able to operate backwards and forwards in time. If the 'out-of-body experience' is a psi effect, then it would apparently demonstrate that the complex mechanisms of the brain, while extremely vulnerable to disruption or total destruction as a result of disease or injury, are apparently unnecessary for perception or cognition in the out-of-body individual. To be fair, some parapsychologists have argued that their data tends to support the idea that the brain does indeed process incoming psi. Yet such processing is not a simple matter, for as Beyerstein (1987) noted, in pointing to the profound implications that psi would have for the neurosciences. He pointed out that perception, memory and emotion involve extremely complex neurochemical configurations that are the result of the spatiotemporal integration of activity in millions of widely-distributed neurons and their internal components. *Extrasensory* perception would by definition bypass the activity of peripheral receptors and nerves that normally determine these central electrochemical configurations. To experience the emotion or the percept, then, any hypothetical 'psi signal' would have to produce the corresponding central electrochemical configurations directly, which would involve influencing the internal chemical processes of millions of neurons in the correct sequences and in the appropriate anatomical pathways. This, in the view of neuroscientists in general, is highly unlikely. Yet while there are attempts to interpret physical theory in such as way as to accommodate psi (e.g. Pallikari in this Issue), parapsychologists appear disinterested in the contradictions between parapsychology and neuroscience (Kirkland, 2000).

On the other hand, failure to jibe with other areas of science is in a very real sense the *sine qua non* of parapsychology. As discussed earlier, something is only considered paranormal if it defies current scientific models of reality.

12. Disinterest in competing hypotheses

Unfortunately, the focus in parapsychology seems to be more on finding the anomaly than on explaining the experience. The retreat into the laboratory has led to a focus on statistical deviations that thoroughly distracts researchers from seeking other, more prosaic, 'normal' explanations, for psychic experiences. That is too bad, for as I have indicated earlier, such experiences warrant study in their own right, regardless of whether there is any need to appeal to paranormal explanations. I have many times argued (e.g. Alcock, 1981) that even if psi does not exist, we should still expect that most people will have experiences in their lifetimes that *seem* to yield to no other explanation than a psychic one, and I have explained just why that should be so and have offered explanations as to how various paranormal experiences can be the result of normal (and sometimes abnormal) brain function. Many other psychologists (e.g. Beyerstein, 1987–8; 1988; Blackmore, 1982; Marks, 2000; Neher, 1990) have also provided substantial and detailed explanations with regard to how normal and abnormal psychological processes are capable of producing all the elements of paranormal experiences. This information should be of great interest, one might think, to parapsychologists, but the question of normal causality seems usually to be dismissed out-of- hand as being unimportant to the study of the paranormal.

Several of the articles in this Special Issue address some of the factors that might explain why some people believe that they have witnessed or experienced paranormal phenomena even if they have not.

1. Ultimately, 'real-life' accounts of paranormal experiences, the very sort of accounts that led to parapsychological research in the first place, rely on processes of perception, interpretation and memory. French provides a good discussion of how such factors as hallucinations, imagery, suggestibility, dissociative tendencies and unreliable memory may be the well-spring of many such accounts.
2. Dean and Kelly are the preeminent critics of astrology and its claims, and they note that astrology actually seems to 'work' for many people — perhaps many millions of people — around the globe. Thus, there is widespread belief in astrology just as there is in parapsychology, and there is a similarity between the two, in that both essentially involve correlations between two events, and the imputation of causality. In parapsychology, one 'wishes' or 'guesses' — either in real life or in the laboratory, and then any significant correlation between wish/guess and outcome is taken as evidence of causality. For example, in a psychokinesis study, a subject wishes to produce high numbers in the output of a random number generator, while in a telepathy study, the subject essentially guesses at what the sender is sending. Similarly in astrology, the astrologer produces a description of one's future, and to the extent that it seems to correspond with what happens later, it is taken to support the notion that the position of the stars at birth is related causally to later events in one's life. Dean and Kelly show that astrology seems 'to work' because of the cognitive errors that individuals make in reacting to

their horoscopes — the fallacy of personal validation. A similar explanation can be applied to the readings offered by psychics. What is particularly important to the discussion of parapsychology in their article is the pervasive extent to which people can come to strongly believe in a demonstrably false system of causality.

3. Brugger and Taylor adduce evidence that supports their contention that believers in paranormal phenomena more readily perceive meaningful associations in random stimuli than do disbelievers, and argue that believers develop an 'illusion of control', perceiving a causal relationship between their actions and environmental events that produces a strong belief in a paranormal causation of the event. They further argue that believers tend not to test alternative hypotheses. To me, their paper is a particularly important one, for it shows the way towards understanding how the exigencies of both everyday life and the parapsychology laboratory can be expected to generate strong impressions that something 'psychic' has occurred.

These are some of the reasons that I would urge caution in one's approach to parapsychological claims. However, no doubt parapsychologists would argue that I am being unfair and overly negative.

This leads me to another question: *Has mainstream science been unfair?* Parker contends that mainstream science has not given parapsychology a fair hearing. I respectfully disagree. I have detailed elsewhere (Alcock, 1987; 1990) how conventional science and mainstream psychology have actually provided numerous opportunities over the years for parapsychologists to bring their work to a larger scientific audience. Indeed, when the American Society for Psychical Research was founded in 1885, its membership included several prominent psychologists of the day, most of whom eventually left the organization when they failed to find any evidence of psychic phenomena. Again, in the early part of the twentieth century, other prominent scientists and psychologists were open to the study of parapsychology, and some undertook studies of their own but gave up when their efforts failed to produce results. In the 1930s, not only did the American Psychological Association sponsor a round-table discussion of parapsychology, but a 1938 poll found that 89% of psychologists at that time felt that the study of ESP was a legitimate scientific enterprise (Moore, 1977). Various scientific publications over the years, including prestigious psychological journals such as *Psychological Bulletin*, have brought parapsychological research and views to the non-parapsychological scientific community. Indeed, between 1950 and 1982, more than fifteen-hundred parapsychological papers were abstracted in the American Psychological Association's *Psychological Abstracts* (McConnell, 1977). Nonetheless, mainstream science continues to reject parapsychology's claims. In my mind, this is not because of some unfair bias, but simply because parapsychologists have not been able to produce data that persuade the larger scientific community that they have a genuine subject matter to study.

This lack of acceptance by science no doubt creates cognitive dissonance on the part of those parapsychologists who are convinced that they do have real

phenomena. This dissonance can be resolved either by assuming that the exclusion from the halls of mainstream science is unfair and unjustified, or that there is some reason other than lack of persuasive data that underlies the rejection. As an instance of the latter, the prominent parapsychologist Charles Tart once wrote that sceptical scientists may be unconsciously so afraid of their own psychic abilities that they have to attack any evidence that might provoke knowledge of their own ability (Tart, 1982; 1984). Parker, in this Issue, argues that perhaps sceptical psychologists do not really want to resolve the issue about the reality of psi for fear of the 'unwanted implications' for psychology if it were shown that psi really does exist. He may be correct, but I doubt it. In my many years in the field of psychology, I have never detected anything other than simple disinterest in parapsychology from the vast majority of psychologists. They simply assume that psi phenomena have never been shown to exist. On the other hand, I am certain that were there suddenly to be produced compelling evidence for the reality of psi, parapsychologists would be knocked over in the stampede by experimental psychologists to explore an exciting new area of research.

Can the psi question be resolved? Parker argues that the technology now exists that would allow a resolution of the question of whether psi exists, and that it would be relatively straightforward to resolve the question, were it not for a lack of funding from mainstream science. He also states that parapsychology might turn out to present genuine phenomena — or, it could turn out to be based on a mixture of fraud, artefact and subjective validation.

I would certainly applaud any effort and investment directed at resolving the psi issue, but I do not think that it is really possible to resolve it, unless of course compelling and replicable demonstrations of the existence of psi are forthcoming. I do not believe that parapsychologists give the Null hypothesis a proper chance, and I cannot conceive of any research that could serve to persuade parapsychologists that psi does not exist. It would be far easier, were good and reliable data available, to persuade sceptics of the reality of psi than to dissuade parapsychologists. What evidence can one produce with regard to 'disproving' the psi hypothesis? Certainly not carefully executed studies that fail to replicate, that fail to produce any evidence of a psi anomaly. Those are too easily explained away in terms of the 'experimenter effect' or simply ignored, as is the case with Jeffers' research. Finding prosaic explanations for a given data set may persuade parapsychologists that, in that particular instance, there was no evidence for psi, but what about all the other data sets yet to come? Parapsychologists can neither tell us under what circumstances psi, if it is real, does not occur, nor can they tell us how it would be possible to disprove its existence.

While some parapsychologists, as noted earlier, ascribe hidden motivations to the continued resistance of mainstream scientists to bring parapsychology into the scientific fold, I judge it unlikely that parapsychologists would under any circumstances abandon their belief in and pursuit of the paranormal. In fact, while Brugger and Taylor propose the joint collaboration of traditional parapsychology and neuroscience in the hope that findings from prospective research conducted by representatives of two apparently conflicting views will most likely be

taken seriously by both sides, they also foresee what many parapsychologists would consider to be an unacceptable downside: 'We thus anticipate that, although psi would vanish from the scene as a process of information transfer, it would live on as a phenomenon of subjective probability worthy of scientific investigation.'

Finally, even if one were to produce a set of circumstances that would lead some parapsychologists to abandon the psi hypothesis, parapsychology as a whole would carry on much as it always has, and the conclusions of those who left the field would be downplayed or ignored, just as were Blackmore's conclusions when she pronounced that she had become sceptical with regard to psi and was leaving the field, or Wiseman's as he had become more and more identified with the sceptical position (Wiseman, 1997). Of course, for those who appropriate for themselves the label 'parapsychologist', but do not really subscribe to the appropriateness of a scientific examination of psi in any case, any agreement by science-oriented parapsychologists that resolves the psi question in a negative direction would carry no weight at all.

Thus, the search for psi will go on for a long time to come, for I can think of nothing that would ever persuade those who pursue it that the Null hypothesis is probably true. Yet, as this search goes on, those of us who are sceptics should applaud and support the approach taken by parapsychologists who have contributed to this Special Issue — not because we agree with their conclusions, for we shall continue to scrutinize and, when appropriate, find fault with their methodology and challenge their interpretations — but because they share our belief in the power of the scientific method to reveal truth in nature. I do marvel at their tenacity, however, for they labour in search of psi despite a lack of the evidentiary and other rewards that are earned by mainstream scientists in their research. Yet, that being said, and as I have stated before (Alcock, 1985; 1987), I continue to believe that parapsychology is, at bottom, motivated by belief in search of data, rather than data in search of explanation. It is the belief in a larger view of human personality and existence than is accorded to human beings by modern science that keeps parapsychology engaged in their search. Because of this belief, parapsychologists never really give the Null hypothesis a chance.

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